and everybody used to call "symbolic logic," M. Poincaré has not been as true to his lofty sentiment as his admirers have learned to expect and demand.

Under these circumstances it seems only fair—I do not mean to logistics but to the public—to give people the opportunity to read M. Couturat's answer as well as M. Poincaré's attack.

In the following translation, any bibliographical or other notes which I have added are enclosed in square brackets. Where possible I have abbreviated the translation and avoided the use of symbols.

There are a few passages in M. Couturat's article which may possibly give rise to a wrong impression. Thus, he speaks of logical demonstrations making true the chance finds of the intellect. Of course the process of demonstration does not do this: It gives the finder and other people certainty as to whether the find is true or not. But we must not accuse M. Couturat of being a pragmatist on the slender grounds of a loosely expressed sentence; especially as in other parts of this article he has protested in the clearest possible way against the confusion between creation and discovery.

Near the end of the second section there is a reference to a number of mathematicians who failed adequately to deal with the paradox discovered by Burali-Forti, among whom are mentioned Russell and myself. The article of Russell referred to contains, implicitly, a criticism of certain views widely held by mathematicians at that time and also—again implicitly—the solution of the paradox and others like it. This of course was familiar to M. Couturat, but the citation of Russell in that connection might mislead some people. With regard to myself, at the time (1903-1904) that I wrote the papers referred to I was hardly, as M. Couturat says, "totally a stranger to logistics," but I freely grant that I was not as familiar with it as is necessary even to grasp the full bearings of the question. My attempt at the solution, though I believe it has one small merit in distinguishing between what may be called *entity* and *existence*, I have since then abandoned.

The discussion, in the third of M. Couturat's sections, of the question of *existence* does not appear to me to be satisfactory, and I have added a note referring to some former remarks of mine on this subject in *The Monist* for January, 1910. P. E. B. J.

REPLY TO M. POINCARÉ.

I thank M. Poincaré for the honor which he has done me by taking me in particular as the subject of his articles

on "Mathematics and Logic,"[3] but I must say that I do not deserve this honor, for the ideas which I have presented are not my own and I fear that M. Poincaré has done them a great wrong by discussing them from a work in which they are given at second-hand. In fact, as I have been careful to warn my readers, my articles[4] were mainly only an account of Mr. Russell's book; and wherever I have been led to add an analysis of the works of other logisticians I have not omitted to refer to them. Now it is not customary to criticize works of this class from a simple analysis of them, above all when the value of these works consists in the rigor of demonstrations, and these demonstrations are necessarily absent from my summary exposition. For example, I have analyzed long memoirs of Peano, Pieri and Whitehead by limiting myself to the enunciation of their chief theorems, without the quotation of a single demonstration. It is constantly assumed that the reader who wishes to see the demonstration of such and such a theorem has only to seek out the original memoirs, and it would obviously be pointless to reproach me for not having given it. Similarly I have thought I ought to describe in my book, to complete one of my articles, Peano's space-filling curve in an elementary and intuitive form which is accessible to the first comer and without speaking of the rigid analytical demonstration. What would one think of a mathematician who, only knowing this curve by my account of it, allowed himself to criticize its construction, to doubt the rigor of the demonstration, or to declare that this demonstration does not exist and that the proposition in question rests on intuition?

Also I had warned my readers that in my work I would

[3] [See Dr. G. B. Halsted's translation of "The New Logics" in *The Monist* of April 1912, and of "The Latest Efforts of the Logisticians" in the present number.]

[4] Published, with some corrections and additions, in a volume bearing the title, *Les principes des mathématiques*, Paris, Alcan, 1905.

sacrifice rigor to clearness, not to that *logical* clearness which is inseparable from rigor and which can only be obtained by logistical symbolism, but to that clearness in the common acceptation of the term which is called *intuition* and which M. Poincaré esteems so highly. It must be granted that I am very badly rewarded for the concessions which I have made to intuition, since M. Poincaré profits by them to reproach me with a lack of rigor. In any case, I wished to do the work of a commentator and a popularizer and to compose for the use of the laity a kind of introduction to the works of which I gave a short account. That is to say, it was not for M. Poincaré that I wrote, and I did not pretend to teach him anything about these works. In all cases, a work of the kind I wrote may serve—I hope so at least—to *teach* the elements of the doctrines in question, but it cannot serve as a sufficient basis to *criticize* these doctrines; to be just and effective, the criticism ought to be on the original works from which I drew my inspiration. What would M. Poincaré say if some one took it upon himself to discuss Hilbert's principles of geometry[5] from the analysis—however exact and complete it may be—which he has given of it to the French public?

I might stop with these remarks, and perhaps I ought to do so; for if I have already compromised the doctrines in question by my attempt at popularization, I run the risk of compromising them still more by undertaking to defend them against an adversary like M. Poincaré. If I have resolved so to defend them, it is, on the one hand, because it has pleased M. Poincaré to substitute me for the masters of logistics, and, on the other hand, because these masters have believed that I would suffice for the task and have left to me the care of justifying them. I thank them for

[5] [*The Foundations of Geometry,* trans. by Townsend. Chicago, The Open Court Publishing Co.]

their confidence; but the reader ought to know that if there is any thing good and enduring in my work it is to those masters that I owe it, and that all that is feeble and defective comes from myself. If then I succeed in justifying logistics against the criticisms of M. Poincaré, so much the better; if not, it will be my fault and will prove nothing against logistics.

I.

In the first place, we must not confuse logistics with what M. Poincaré calls "the logic of M. Hilbert." M. Poincaré has not made this confusion, but many of his readers may do so when they see him associate these two doctrines in the same discussion and in a common condemnation. Now it must be clearly realized that Hilbert is a complete stranger to logistics and has never used any logical calculus in his researches. If then the criticisms that M. Poincaré makes against him are just, they have no bearing against logistics, but rather tend to prove the superiority of logistics over verbal reasoning and simple common sense.

It is important also to correct a historical error to which the following phrase of M. Poincaré may give rise: "What Hilbert has done for geometry others wish to do for arithmetic and analysis." We might believe from this passage that the logisticians attack the subject of arithmetic and analysis *after* the works of Hilbert on geometry, and in imitation of them. The *Grundlagen der Geometrie*[6] of Hilbert were published in 1899. Now, ten years before this (in 1889) Peano had published not only his *Arithmetices principia nova methodo exposita* but also *I Principii di Geometria logicamente esposti,* both of which were written in the symbolism which he had invented in the year before. In 1891 he published in the first volume of the *Rivista di Matematica* two articles on the concept of num-

[6] [English translation as noted above.]

ber which already contained the five fundamental axioms of arithmetic. In 1894 he published in the fourth volume of the same *Rivista* the memoir on the foundations of geometry which I have analyzed in *Les Principes des Mathématiques.*[7] Lastly, in 1899 Pieri published his logical reconstruction of projective geometry and of metrical geometry in the Memoirs of the Turin Academy. These dates are enough to prove that, if Hilbert has not *wished* to profit by the works of the logisticians, the logisticians *could* not have profited by his work and had no need of his example not only in arithmetic and analysis but even in geometry. Consequently M. Poincaré commits a historical error in attributing to the "works of M. Hilbert" the "triumph" of logistics in geometry. I content myself, on this point, with stating a fact: In 1900 Hilbert elaborated for arithmetic a complicated system of *eighteen* axioms,[8] when *eleven years before this* arithmetic had been built up on *five* axioms only, which Padoa in 1902 reduced to four. Finally, to render to each person the "chronological" justice which is due to him, I should record that Frege stated, in his *Grundlagen der Arithmetik* of 1884, the theory of the integer number which Russell has adopted in principle, and undertook to prove that the principles of arithmetic are purely logical—analytical in Kant's sense.

M. Poincaré writes: "This invention of M. Peano was called *pasigraphy,*" and adds: "This name exactly defines its bearing." The first phrase contains an error of fact. *Never* did Peano call his logical symbolism by the name of "pasigraphy"; he always called it "mathematical logic".[9] If I call it "logistics," it is first, because of the equivocal-

[7] Chap. VI, pp. 159-180.

[8] "Ueber den Zahlbegriff," *Jahresber. der deutsch. Math.-Ver.*, Vol. VIII, 1900. [This essay was reprinted in an appendix to the 3d German edition of the *Grundlagen der Geometrie*, Leipsic and Berlin, 1909, pp. 256-262.]

[9] See all the editions of Peano's *Formulaire de Mathématiques*, and the *Notations de Logique mathématique* (Turin, 1894) which forms the introduction to the first edition.

ness of the expression "mathematical logic," and, secondly, not because "this new name implies the purpose of revolutionizing logic," but because this good old word, which Vieta gave to algebra, indicates, by its very etymology, the general art of reasoning and calculating. In this sense it was employed in the eighteenth century by Lambert to denote his own logical calculus.[10] It was Schröder who first called it "pasigraphy" in a communication made to the first congress of mathematicians at Zürich in 1898, and that probably with a depreciative intention.[11] Now this word is quite inexact, whatever M. Poincaré may say. People call any written universal language a "pasigraphy"; thus the international code of maritime signals[12] is a "pasigraphy." I myself formerly used this word when speaking of Peano's symbolism but I corrected it at once: "A system of *pasigraphy* or, better, of *ideography*";[13] this means that the symbols translate not words or phrases but ideas. I concluded the same article by the words: "We would restrict incorrectly the value of Peano's symbolism if we only regarded it as a kind of stenography. It is also and chiefly an instrument of logical analysis, of deduction and of verification"; and I recalled, *à propos* of this, the "universal characteristic" of Leibniz. It is, then, entirely to misinterpret the nature and bearing of logistics to regard it as a mere pasigraphy.

For the rest, M. Poincaré speaks of logistics in the way in which a *bel esprit* might speak of algebra or mathematics in general. For example, he says: "The essential elements of this language are certain algebraic signs which represent the different conjunctions *if, and, or,* and *then.* That these signs may be convenient is possible, but that

[10] "Versuch einer Zeichenkunst in der Vernunftlehre, *Logische und philosophische Abhandlungen,* edited by John Bernoulli, Berlin, 1782.

[11] Translated into English in *The Monist* for October, 1898.

[12] Cf. Couturat and Leau, *Histoire de la langue universelle* (Paris, 1903), preliminary chapter on "Les pasigraphies."

[13] *Bulletin des Sciences mathématiques,* Vol. XXV, 1901.

they are destined to revolutionize the whole philosophy is another question. It is difficult to admit that the word *if* acquires, when it is written $\supset$, a virtue which it did not have when it was written *if*." In the first place we must not believe that logistical symbols are limited to the literal translation of some words.[14] The sign $\supset$ translates *if* no more than *then,* it expresses the idea of implication; the same sign may translate *and* in certain cases and *or* in other cases. Inversely, the word *and* has not the same meaning in the three following cases: "Peter is rich and happy," "Peter and Paul are rich," "Peter and Paul are brothers"; and consequently it is not translated by the same logistical symbol. It is, then, quite unjust to consider "the new language" as a mere tracing of ordinary language and consequently as having no value and no utility of its own.[15]

M. Poincaré believes that I attach "an exaggerated importance which would astonish M. Peano himself" to the use of symbols. I can reassure him on this point. M. Peano writes to me on this subject: "I have always affirmed the importance of symbolic notation in all mathematical propositions, its great utility in difficult and delicate questions, and its indispensability in the study of principles. That is written down in all the volumes of the *Formulaire* ...."Everywhere and always he insists upon the necessity of expressing every mathematical proposition and every definition *entirely in symbols.*[16]

[14] Like the childish notations of Hérigone, who wrote, for example, "5<" for *pentagon*; or like any system of analogous abbreviations that a mathematical student may invent for taking notes.

[15] In 1895 Peano wrote: "Mathematical logic....does not reduce merely to an abbreviated symbolical writing, to a kind of tachygraphy; it allows us to study the laws of these signs and the transformations of propositions.... The two objects of mathematical logic, the formation of a symbolical script and the study of the forms of transformations (or of reasoning) are closely connected" ("Sur la définition de la limite d'une fonction," *American Journal of Mathematics,* Vol. XVII). This memoir was meant (as its subtitle "Exercice de logique mathématique" shows) to make the new logic known to mathematicians. Mathematicians then cannot be excused for still ignoring it, and it is doubly inexcusable for them to criticize it without knowing it.

[16] Cf. his memoir printed at Paris in 1900 among those read at the first international congress of philosophy.

However that may be, there was some one who had an opinion which is as "exaggerated" as that of Peano and myself of the importance of symbolism, and that was Leibniz. He went as far as to say that the discoveries in mathematics that he had made arose solely from the fact that he had perfected the use of symbols, and his discovery of the infinitesimal calculus was, for him, only a specimen of his *characteristica universalis*.[17] In fact, we know that he did not invent infinitesimal *ideas*; he only invented a symbolism to represent them and an algorithm to manipulate them. We might say of him: "He only introduced two new signs, d and $\int$. That these signs may be convenient is possible; that they could revolutionize the whole of mathematics is incredible." We might also say of algebra: "It consists simply in representing by signs the words *plus, minus, multiplied by,* and *divided by.* But it is not to be seen how it constitutes a progress beyond arithmetic; it is difficult to admit that the word *plus* when it is written $+$ acquires a virtue that it did not possess when written *plus*." And yet, could the theory of equations and the theory of algebraic forms have been elaborated with *words*?

M. Poincaré asserts that "pasigraphy does not preserve us from error." Without doubt it does not, any more than the rules of algebra or arithmetic do. Does it follow that these rules are false or that we ought to defy them? Because we make mistakes in addition, must we condemn the four rules of arithmetic and even the arithmetical signs, and only count on our fingers or with little balls? The mistakes which a logistician may commit do not weaken the value of logistics any more than mistakes in calculation shatter the certainty of arithmetic. It is enough that logistics allows us to reason more easily and more surely and to discover faults of reasoning more easily; and that

[17] See Couturat, *La Logique de Leibniz*, pp. 84-85, the texts cited in the note and the third appendix.

is what it does. In this sense it is, as Leibniz said, an art of infallibility—not that logisticians are infallible, but they are less exposed to error than those who trust to simple common sense, that is to say to intuition.

Besides, M. Poincaré forms quite a false idea of logistics by considering it as a mechanism from which intelligence is nearly excluded; and his comparison of it with the "logical piano" of Stanley Jevons is not exact. We must first of all know that this logical piano merely concerns logical classes and that it only effects the least important —and the most mechanical—part of reasoning. Its office consists in suppressing the elementary classes which are annulled in virtue of the given premises. But it leaves almost all the rest to be done; thus, on the one hand, we have to put the logical problem into equations, and, on the other hand, we have to combine the subsisting classes in such a way as to obtain the consequences in the desired form. Thus the algebra of logic does not reduce to a blind mechanism. This is still more true of logistics which surpasses the algebra of logic and is much less "mechanical."

Another comparison is no happier: "Are the rules of perfect logic the whole of mathematics? We might just as well say that the whole art of the player of chess reduces to the rules for moving the pieces." But nobody ever asserted that all mathematics reduces materially to logic, that is to say that there is *nothing more* in a treatise on mathematics than in a treatise on logic. We maintain only that all mathematical reasonings are effected in virtue of the rules of logic *alone,* in the same way that all the games of chess that have been and can be played are effected according to the rules of the game. . . . , otherwise the rules would be worthless. The comparison returns then against the adversaries of logistics, for it shows how a small number of elements, combined according to some few fixed laws, can generate an unlimited variety of consequences. People

have asserted that logistics put leading-strings on invention, and have urged against logistics the rights of genius. How could mathematics constantly evolve and progress if it is always condemned to rest on a small number of principles and "logical constants"? M. Poincaré does not use this argument and leaves on one side the question of invention; but it is clearly visible that the theory of "logical constants" inspires in him an instinctive repugnance, and that every attempt to catalogue the primitive notions and principles of mathematics appears to him to be an insupportable pretension and a restriction on the "liberty" of the scientific man. It is for that reason that he opposes to logical and demonstrative reason the "sure instinct" of the inventor and the "more profound geometry" which guides him; and these kinds of considerations are very much in fashion. It is, at the present time, fashionable to put the "logic of nature and of life" in opposition to formal logic that is disdainfully called "dialectical," "abstract," and "verbal."

There is here a confusion which it is important to dissipate. To oppose to logic the psychological fact of invention is to commit the most gross *ignoratio elenchi.* Logic has neither to inspire invention nor to explain it; it contents itself with controlling it and *verifying* it in the proper sense of the word (making it true). Do we reproach metrical science for not giving poetical genius or the science of harmony for not conferring musical genius? And do we therefore conclude that the rules of both have no value and no utility? As for the theory of "logical constants," the liberty of the mathematical discoverer is no more restricted by formulating the primitive principles and notions on which his science rests than the libetry of the musician, of the painter and of the poet is restricted by saying to them in turn: "As for you, you will never be able to do anything but combine the seven fundamental notes with

their accidentals; as for you, the seven colors of the spectrum, and as for you, the twenty-six letters of the alphabet." That is exactly in what measure logistics clogs invention and clips the wings of genius. People should really stop throwing invention at the head of logicians, as if invention could be contrary to logic. Besides, this "sure instinct" and this "more profound geometry" which guide the discoverer are only unconscious forms of the logical reason and proceed according to the same laws. The reason which invents is conformable, and at bottom identical, with the reason which demonstrates, and without it the latter could not *verify* what the former has by chance found; and these chance finds are only *true* on this condition. It is, then, conformity with the laws of logic "which alone gives value to the edifice which has been built."

M. Poincaré speaks of "the logic of Russell" and opposes it to the logic of Aristotle, as if Mr. Russell was the first to go beyond the confines of the Aristotelian logic. He appears besides to have an inexact notion of the Aristotelian logic when he says: "The logic of propositions of Russell is the study of the laws according to which the conjunctions *if, and, or* and the negation *not* are combined. It is a considerable extension of the ancient logic." I can assure M. Poincare that Aristotle was already acquainted with the conjunctions *if, and, or* and negation, and that he took account of them in his logic. All the classical logicians knew and studied hypothetical judgments (where *if* figures), copulative judgments (where *and* figures), and disjunctive judgments (where *or* figures); and classical logic has always admitted *negative* judgments. If M. Poincaré means that Mr. Russell is the first who has translated these judgments into symbols and submitted them to an algorithm he is at least half a century out of his reckoning: for it is to Boole (without speaking of his fore-runners)

that this honor is due. It is, then, not Mr. Russell who has "adjoined" to syllogistics "the conjunctions *and* and *or*" and who has thus "opened up a new domain to logic."

M. Poincaré believes that he can establish a fundamental difference between the two logics by remarking that "the symbols are multiplied and permit of varied combinations *which are no longer finite in number*," and he adds: "Have we any right to give this extension to the meaning of the word *logic*?" It would, then, seem that for him logic is characterized by the *limited number* of the combinations which it admits. But I do not see that there is a radical difference. Besides, in what sense did the ancient logic only admit a limited number of combinations? Is it a question of the number of valid moods of the syllogism? But modern logic, too, only admits a limited number of simple types of reasoning. Is it a question, on the other hand, of the infinite diversity of complex reasonings that one can obtain by combining these types? But classical logic too could form an infinity of reasonings by combining syllogisms. In all cases the two logics have the same character and only differ in respect of the more or less. Besides, how is the number relevant in this matter? If a logical principle is true, whether it be the principle of the syllogism or any other, is it not capable of justifying an infinite number of reasonings just as well as a finite number? Does its demonstrative virtue by some chance become exhausted after n applications? Lastly, what means this reproach addressed to logistics of admitting an infinite number of combinations, when, on the other hand, it is reproached with only having a very limited number of principles? Is it not, rather, for it, just as it is for geometry (according to a well-known phrase), a glory to deduce from so small a number of principles so great a number of consequences? How can this fact scandalize a mathemati-

cian who is familiar with the incredible fruitfulness of the theory of combinations?

When M. Poincaré opposes the old and the new logic to one another and considers the latter as an enormous and perhaps illegitimate "extension" of the former, he appears to forget the fact that the *domain* of a science may receive an extension—even a considerable one—without the *notion* and the definition of this science changing. Otherwise we could never speak of the progress of the sciences: M. Poincaré seems to suppose by that that a science remains in essentials identical with itself in the course of its historical development. The reasoning of M. Poincare would serve to prove that the infinitesimal calculus is not a part of mathematics; that electricity is not relevant to physics, and that the theory of organic compounds is not relevant to chemistry. Now it is for this reason that the extension of the "field" of classical logic becomes an extension of the "meaning of the word *logic*." M. Poincaré says again: "It seems that there is nothing new to write about formal logic and that Aristotle saw to the bottom of it." If he means by that (as Kant did) that logic has made no progress since Aristotle, it is nowadays a simple error of fact; but if he means that logic ought to remain (or ought to have remained) confined in the domain assigned to it by Aristotle, he maintains implicitly that logic was perfect and complete at its birth, and this is contrary to the analogy of all the other sciences and to probability. We would only smile at a man who claimed to reduce mathematics to what it was in the time of Euclid, and physics to Aristotle's physics. How then dare any one maintain or insinuate that Aristotle has said the last word about logic and that it is forbidden to develop this science beyond the narrow limits assigned to it by its founder?

Besides, if "the new logic is richer than the classical logic," it is not so much by the extension of its domain as

by the deep study of the principles that have *always* directed those reasonings which have been recognized as just by that rational instinct to which M. Poincaré attaches so much value. He seems to reproach the logisticians with "introducing" into logic indefinable notions and indemonstrable principles. It would be more just to say that they have discovered or recognized them; just as Aristotle did not invent but discovered and recognized the principle of the syllogism. M. Poincaré is in too great a hurry to assert that these indemonstrable principles "are appeals to intuition, are synthetic *a priori* judgments." Perhaps he would have been of another opinion if he had taken the trouble to run through the enumeration of these principles. Why should the principle of composition: "If a is b, and a is c, then a is bc" constitute an appeal to intuition rather than the principle of the syllogism: "If a is b, and b is c, then a is c"? In what is the principle of simplification: "ab is a" more synthetic than the principle of identity with which it has been so often confused? In any case, it has been considered by Kant as the type of analytic judgments. Is it of these principles that M. Poincaré said: "We regarded them as intuitive when we met them, more or less explicitly enunciated, in treatises on mathematics. Have they changed character because the meaning of the word logic is enlarged and we now find them in a book called *Treatise on Logic*"? In what treatise of mathematics has M. Poincaré seen them formulated? And his argument returns on himself, for even if they were put in a treatise on mathematics, would that change their character as logical principles? "*They have not changed their nature, they have only changed place,*" writes M. Poincaré in italics; but it is he who has changed place. It is not enough that they should be used in mathematical reasonings to call them mathematical, and it is not enough that they are not found in treatises on classical logic to refuse to them the title of

logical principles. Otherwise it would be necessary to say that *logical* principles are, by definition, those which Aristotle and the schoolmen have discovered and formulated; and that all the logical principles discovered by modern logicians are *intuitive*. The distinction of the logical and the intuitive would then reduce to a question of chronology.

Besides, the vague conception of *intuition* is out of place as a weapon against the logicians, especially when the intuition spoken of is not specified. Is intellectual intuition meant, which bears upon the relations of ideas, or sensible intuition, which necessarily clothes the spatial form? These two intuitions are wholly different. All logicians are ready to recognize that their principles proceed from intellectual intuition, that is to say they are objects of immediate knowledge by the reason; but very few will agree that they proceed from sensible intuition, and rest, for example, as Lange has maintained, on spatial schemata. For the rest, whatever the solution of this "metalogical" problem may be, all the logical principles ought to have the same fate; and the traditional principles of identity, contradiction and so on will be "appeals to intuition" in the same sense and in the same measure as the others. The logisticians then, must not be accused of altering logic by introducing intuition into it; for if this accusation has any value, it is Aristotle who began this introduction.

In any case it is inexact to say that "living" reasonings, the only ones "in which our mind remains active," are "those in which intuition still plays a part." Purely logical reasonings need more mental effort and ingenuity than M. Poincaré believes, and, even with the mediocre aid of Jevon's logical piano, a certain cleverness is necessary to combine the brute results of mechanism and to draw the conclusion wished. Besides, why reproach logistics with

making reasonings easier and more sure?"[18] If, like algebra, it condenses into short formulae the result of long and complicated reasonings, it is to relieve the powers of the mind and to allow it to embrace a greater number of data and to draw vaster and more distant conclusions. Consequently, far from paralyzing the faculty of invention or rendering it useless logistics lends it stilts or wings. The discovering mind will always find something to exeroise itself upon, but it will do so on data which are more and more complex. That is what happens in analysis, where each new theory combines formulae which sum up the results of simpler and more elementary theories. M. Poincaré may then be reassured: logistics does not exclude genius.

M. Poincaré makes a curious reproach to logistics: "The part of intelligence is restricted to choosing among a limited arsenal rules posited beforehand, and has not the right to invent new ones." If we remark that the "rules" are none other than the principles of logistics, this phrase appears to me to mean that intelligence "has the right" to invent new logical principles. It is a strange conception of logic to consider it as always evolving and as never finished.[19] It evidently proceeds from the psychological confusion between the science and what we know of it at a given moment. No one will ever "invent" new rules of logic; some of these rules which had not been noticed but were quite as "ancient" as the others and equally "posited" beforehand, that is to say *a priori*, will perhaps be "discovered." And the logisticians do not do anything else. But then, why does M. Poincaré reproach

[18] "More sure," for M. Poincaré confesses that in living reasonings "it is difficult not to introduce an axiom or postulate which is unperceived." Must we conclude from that that "life" is incompatible with logic?

[19] To use the favorite comparison of M. Poincaré, what would we say of a chess-player who wished to invent a new rule in the middle of a game,—for example, to make his king move several squares when in check? Such an "invention" would be called trickery and nothing else.

them with innovating? With respect to the nine indefinable notions and the twenty indemonstrable propositions of Russell, he says: "I believe that....I would have found some more." He is quite at liberty to do so: the logisticians do not ask for anything better, and will register his discoveries —or, if he prefers to say, his inventions—with gratitude. But what do these contradictory reproaches mean if not that M. Poincaré claims for himself "the right" to "invent" logical principles at the very moment when he refuses this right to the logisticians?

For the rest, what good is it to discuss *in abstracto* the qualities of logistics? M. Poincaré grants that "pasigraphy can furnish us with a criterion to decide the question which occupies us. If every treatise on mathematics can be translated into the Peanian language, the logisticians are right." Now the logisticians replied in advance, long ago, to this ironical invitation. Ten years ago Peano published the first edition of his *Formulaire de Mathématiques*, which is precisely a treatise or manual entirely written in logistics; the fourth edition (1903-1904) comprises Logic, Arithmetic, Theory of Numbers, Algebra, the Theory of Real Numbers, the Theory of Definite Functions, the Infinitesimal Calculus, the Theory of Complex Numbers, the Theory of Circular Functions, the Geometrical Calculus (comprising the theory of vectors and the theory of Quaternions), and Differential Geometry; the "Additions" even contain the elements of kinematics. The fifth edition of the *Formulaire* is in course of publication.[20] The principal theorems are accompanied by their logistical demonstrations. I will add that this mathematical manual is a collective work which M. Peano and his collaborators are incessantly revising and perfecting. Consequently the proof

[20] Professor Peano has published, besides, a classical manual entitled: *Aritmetica generale e algebra elementare,* drawn up in logistics (Turin, 1902).

which M. Poincaré requires of logisticians was given long ago and is being completed from day to day.

It is true that M. Poincaré soon seems to repent of his rash concession and adds: "Again we must examine the translation closely. It is not sufficient that we should be presented with a single page where there are only formulae and not a single word of ordinary language, in order that we must bow down....It will be necessary, when we are in the presence of a pasigraphical reasoning, even when this reasoning is correct, to examine if an appeal to intuition is not hidden away in some corner." These reserves are evidently very just in so far as they are counsels of critical method. But why does M. Poincaré not conform to them? It is not enough to express these general reserves which are applicable to any demonstrative work, to weaken the value of logistics and throw disfavor and suspicion on the work of logisticians. The logisticians have given to the public not "one page" but more than three hundred pages of logistical formulae and demonstrations. Let those who have doubts on the value of these demonstrations "examine" them as closely as they wish and let them point out lacunae and errors,—for that is their right and even their duty. But the burden of proof falls on them, and it is not enough, in order to get rid of this burden, to shake their heads with a smile of incredulity.

II.

I pass on to the objections aimed at logistics in so far as it is applied to mathematics. Here again I must say that M. Poincaré wrongs it by judging it merely from the "popular" exposition which I have given of it. In effect, the logistical formulae which constitute, as M. Poincaré says, a "new language" are sufficient by themselves and are intelligible wholly by themselves; if it were necessary to add to them a single word of ordinary language, it would

prove their incompleteness or defectiveness. Besides, this "new language" was invented expressly to avoid the equivocation or the beggings of the question implied more or less confusedly in ordinary language. Consequently the logistical formulae are the *only* ones which can be exact, rigorous and exempt from the above logical vices. Thus, when an author thinks that he ought to translate them into ordinary language, it is merely to make them more accessible to the "laity"; but it must be understood that this verbal translation is always imperfect, approximate and by no means allows the proper appreciation of the logical value of the formulae. Just because language cannot equal the precision and the rigor of the formulae, I have made no scruples about introducing into my verbal translations apparent beggings of the question in order to make them more clear and more "French." What does an inexactitude more or less matter when the logistical formula alone counts from the logical point of view? I could not expect that any one should judge and condemn these formulae from the mere inspection of the verbal translation which I gave of them for the use of novices. All translation is a betrayal; but that is still more true when the translation makes exactly those qualities of the original on which study and discussion bear vanish. It is exactly as if some one wished to study the meter of Virgil in a French translation of the *Aeneid.*

Now, it is of these verbal translations, *and only of these verbal translations*, that M. Poincaré has taken account in his criticism; he does not appear to have noticed the logistical formulae; "It is Greek, so it is unread." He may then "amuse himself by counting how many numerical adjectives my exposition contains": that will prove absolutely nothing against "pasigraphy." Nevertheless we will examine his arguments one by one in order to show better that they all miss the point. On the subject of the logical

definition of zero, he says: "to define *zero* by something *null* and something *null* by *none* is indeed to misuse the richness of the French language." Then he recognizes that I have "introduced an improvement in *my* definition" (a double inexactitude, for this definition is not my own and the "improvement" in question is due to Mr. Russell) by writing "what," according to M. Poincaré "means, in French, zero is the number of objects which satisfies a condition which is never satisfied. But as '*never*' signifies '*in no case*' I do not see that the progress is very great." I will confine myself to recalling the verbal translation that I have given of this formula: "if φx is always false, Λ is the class of x's which verify φx." The verbal translation of that is: Λ is the class of objects which satisfy a condition which is *always false,* that is to say, false for *all* the values attributed to x. Where is to be seen in this formula the idea of the number zero or even of any number? And are we to be reproached for introducing into logic mathematical notions, when classical logic was acquainted with universal judgments and used the word *all*? To be able to attribute to us a begging of the question—even one that is simply verbal—M. Poincaré has had to transform our translation by replacing "always false" by "never true." If, then, somebody here abuses the French language it is not I.

But this reproach is even more undeserved if it is addressed to the logistician, who writes neither in French nor in Italian nor in English but in a symbolism made expressly to liberate ideas from the tacit implications that language introduces into them by custom. M. Poincaré himself says: "It is impossible to give a definition without enunciating a phrase and difficult to enunciate a phrase without putting in it a name of a number or at least the word *many* or a word in the plural.[21] And then the roof

[21] On the subject of the plural, it may be remarked that Peano has, follow-

is slippery and at every moment there is a risk of falling into a begging of the question." These very just reflections bear only on the logical defects of language and on the faults that language can make us commit. It is precisely to avoid these faults and to cure these defects that the logisticians have invented their rigorously defined signs which have no meaning but that which they are given by definition.[22] Put shortly, M. Poincaré's argument comes to this: "All those who reason with the words of ordinary language are *liable* to commit beggings of the question; now the logisticians use, not words, but symbols rigorously defined; consequently they too *must* commit beggings of the question." The syllogism is not conclusive for it has four terms. And even if it had only three, that is to say when one could legitimately conclude from words to symbols, the two words which I have italicized would still render it invalid; the major says that we *may* commit errors; the conclusion asserts that certain authors have *necessarily* committed them.

The criticism of the definition of the number 1 is no firmer. "*One* is the number of elements of a class of which any two elements are identical"; such is the verbal translation that M. Poincaré gives of this definition. "It is more satisfactory....in the sense that, in order to define 1, we do not use the word *one*;—but still the word *two* is used"; and M. Poincaré rightly suspects that *two* can only be defined by means of *one*.[23] But he makes an unjust use of the fact that I have used the word *two* to make a phrase in ordinary language. The more exact translation of the logistical formula is: "1 is the class of classes u which are

ing Leibniz's recommendation, excluded it from the "uninflected Latin" which he has given out as a form of international language, and which has been adopted by many.

[22] Cf. the beginning of the preface to Peano's *Arithmetices principia* of 1889.

[23] By means of the general formula by which we define $n+1$ by means of n; cf. *Les Principes des Mathématiques*, Chap. II, § B, p. 59.

not null and such that if x is a u and y is a u then x is identical with y whatever x and y may be." Where is there, I do not say the word, but the idea of two in this formula? M. Poincaré will say perhaps that *two* (problematic) elements x and y of the class u are made to appear in it; but the fact that they are *two* does not come into the question in any way; and the proof of it is that in reality they are only *one*: x and y are merely two names (excuse me, *names*) for a single individual. This criticism obviously has no bearing on another equivalent formula which I have given,[24] and which may be translated: "*One* is the class of classes u which are not null and such that if x is a u the class of the elements of u which are not identical with x is null." That presupposes of course the definition of the null class; but, as we see, there is no more even a problematical *two* elements of u, but only *one*, and we only express that there is no other.[25]

Will anybody say that, by the mere fact that *an* element is spoken of, the number *one* is implied?[26] But that is an objection which M. Poincaré does not formulate and to which I have replied in advance in the following passage: "We must not believe that the definition of the number *one* constitutes a vicious circle, for the definition of the singular class rests solely on the relation of identity. If it is true that it implies in a sense the *unity* or rather the *individuality* of the element considered, this unity cannot be identical with the *number one* which is to be defined: for this unity is a property of each element while the *number one* is the property of a class....consequently, in all cases

[24] *Ibid.* I have logistically deduced this from the preceding one on p. 60.

[25] Here is a more fundamental definition that Mr. Russell has communicated to me: "*One* is the class of classes u such that the proposition: 'x is a u' is equivalent, for all values of x, to 'x is identical with c' is not false for all values of c.'" Notice that this definition does not presuppose the notion of the null class. As for the formula "x is a u", cf. its definition farther on.

[26] [In French, the same word *un* stands for both *an* and *one*].

the *units* which constitute a cardinal number are different from the number *one*."[27]

The confusion which exists in many minds between these two ideas arises, I believe, from the double meaning of the word for one, which is used both as the name of a number and as an indefinite article.[28] In the latter case it would be better to use the word *some* as the logicians do.[29] This equivocalness exists in French and German, but not in English. If then, somebody is inclined to invoke it, he should take care to abuse not "the richness" but the poverty of the French language. To sum up, it is not enough to conceive *any one* object to conceive the *number one,* nor to think of two objects together to have by that alone the idea of the *number two.* From the fact that a logical formula contains two or many symbols we must not conclude that it implies by that alone the idea of two or of some other number. When we say: "Peter and Paul are wise," we mean to say: "Peter is wise and Paul is wise"; we do not think the *number two* and we have no need to think it nor to notice that that makes "two wise men." In the same way when we say: "x and y are elements of the class u,"[30] we do not think the number two and no idea of number is implied in this proposition.[31]

[27] *Les Principes des Mathématiques,* Chap. II, § A, pp. 47-48. M. Poincaré seems to propose or to accept such a justification when, after having quoted the phrase of Hilbert: "Let us consider the object 1," he adds: "Remark that by doing this we by no means imply the notion of number, for it is understood that 1 here is only a symbol. . . ." Doubtless, but it is *a* symbol, that is to say *one* object. Will M. Poincaré say that that implies the *number one?* Or will he grant to the logisticians the same liberty as to Hilbert?

[28] [Cf. note 26].

[29] And also M. Méray, thus giving example in logic to other mathematicians.

[30] Notice that it is only grammar which makes us use the sign of plural in *are elements.*

[31] Here is the rigorous definition of the proposition "x is a u," that Mr. Russell has communicated to me: "x is a u" means: "The proposition: 'ϕx is true, and u has the relation of a class to the property which defines it' is not false for all values of x." There is not here the shadow of the idea of the number one, but, as in my enunciation, the purely logical notions of *false, negation* and *all.* This definition was already given by G. Frege, *Grundgesetze der Arithmetik,* Vol. I, 1893, p. 53.

These considerations reply to this objection of M. Poincaré's: "A relation is incomprehensible without two terms; it is impossible to have the intuition of the relation without having at the same time the intuition of its two terms." That proves nothing, and M. Poincaré adds: "And without remarking that they are two, for in order that the relation may be conceivable, it is necessary that they should be two and two only." It is not the question to know—and it is a psychological question—if we "remark" or not that they are two, but if the notion of the relation implies that of the number *two*. Now for that it would be necessary that it implied the notion of the class formed by its "two" terms; and that is obviously not the case. The proposition: "x is the father of y" by no means implies the idea of the class formed by x and y. Besides, it often happens that a relation (which is then called reflexive) exists between a term and itself. Would it be maintained then that it has still two terms? That would be to say that x is at the same time *one* and *two*.

The only logistical formula that M. Poincaré has criticized in itself and not in its verbal translation is one given by Burali-Forti. M. Poincaré says on this point: "I understand the Peanian language too little to dare to risk a criticism." This confession would disarm us if he did not "risk" this criticism immediately afterwards: "I fear that this definition begs the question, for I see the figure 1 in the first member and '*Un*' in the second member." M. Poincaré has trusted too much to his "intuition," and it has deceived him. Instead of "risking" this criticism on the mere witness of his eyes, he ought, conformably to the fundamental rule of mathematical method, to have substituted for what is defined the phrase which defines it; and to ascertain if this definition really begs the question, he had only to refer to the definition of the symbol "*Un.*"

Now M. Burali-Forti defines "*Un*" as the class of sin-

gular classes, which in Russell's definition of the *cardinal number 1*. This definition is equivalent to the one which I have given above and neither of them implies the idea of that which is defined. As to the formula which M. Poincaré has criticised, it means: "1 is the ordinal type of the ordered classes of which the cardinal number is unity." Thus it consists in defining the *ordinal* number 1 by means of the *cardinal* number, and this explanation is enough to do away with any appearance of a vicious circle. So we see how "risky" the criticism of M. Poincaré is.

He seems to consider as insignificant the formula

$$1 \, \varepsilon \, \mathrm{No}$$

which M. Burali-Forti deduces from his definition. M. Poincaré translates it inaccurately as: "One is a number"; and then makes merry at the expense of pasigraphy, which "is sufficient to demonstrate that one is a number." If he had read the memoir of M. Burali-Forti—even in the "interlinear Italian translation"—more attentively he would have known that "No" means ordinal number, and perhaps he would have found the formula which teaches us that 1 is an ordinal number less ridiculous. Even if this formula "taught" nothing to M. Poincaré, he had no grounds for judging it to be insignificant, and that for two reasons. On the one hand, this formula is sufficient to prove that the class "No" exists, and this result is not to be despised, since M. Poincaré attaches so much importance to existence-theorems and wrongly reproaches the logisticians with neglecting them. On the other hand to prove that all the finite whole numbers are ordinal numbers, we are obliged to use the principle of induction, and for that purpose to set out from the fact that 1 is such a number. However evident or trivial this fact may appear to M. Poincaré, it was important to demonstrate it, and the formula at which he mocks proves the conscientiousness and the rigor of the

logisticians. The pleasantries of M. Poincaré are then quite pointless.

As for the paradox discovered by M. Burali-Forti in the theory of transfinite ordinal numbers, and from which M. Poincaré deduces an argument against logistics, I will only say that this contradiction can by no means be imputed to the use of logical symbols; and the proof of this is that mathematicians who are total strangers to logistics recognize it, discuss it, and have for years past spent vain efforts to solve it.[32] It is a purely logical difficulty which resides in the principles of the logic of classes, that is to say in the old and traditional part of logic. M. Burali-Forti, in a communication made to me,[33] believes that the contradiction arises from the different meanings that are given to the word "ordinal number," and that it depends, at bottom, on the extension and the properties attributed to the concept of *class*. Mr. Russell believes that it can only be solved by restricting or even sacrificing the notion of *class*; broadly speaking, we must give up the principle—apparently so evident and clear to intuition—that each concept determines a class which is its extension.[34] If logistics has enabled us to discover this contradiction, it can only be considered as a merit and not as a reproach for it proves that it is an instrument of precision for thought. But M. Poincaré is more exacting. He summons logistics to re-

[32] Bernstein, *Math. Ann.*, Vol. LX; Jourdain, *Phil. Mag.*, 1904-1905; Russell, *Mind*, 1905; Hadamard, Borel, Lebesgue, Baire, *Bulletin de la Société Math. de France*, 1905; Zermelo, Borel, König, Schönflies, *Math. Ann.*, Vol. LIX, LX.

[33] [Cf. for fuller details pp. 228-229 of Couturat's original paper].

[34] See Russell, "On some difficulties in the theory of transfinite numbers and order types," *Proc. Lond. Math. Soc.* (2), Vol. IV, 1905, pp. 29-53. M. Poincaré concludes hastily: "Burali-Forti and Cantor have arrived at contradictory conclusions; thus one or the other is mistaken." It cannot be said that one of them is mistaken if it is a question, as Russell shows, of a contradiction of principle, of a kind of antinomy. Thus we can see how much the conclusion is worth: "consequently pasigraphy does not preserve us from error." For the rest, logistics is only a "method of infallibility" (as Leibniz said) if certain premises are granted; it cannot be made responsible for a contradiction inherent in the premises.

solve here and now the contradiction which has become the crux of mathematicians. He says of Mr. Russell and Dr. Whitehead: "If they could have....purged it [the theory of infinite numbers] of every contradiction, they would have rendered us a signal service." The logisticians are not obliged to solve difficulties which stop all mathematicians, M. Poincaré included, and—as if they were modern Oedipuses — to reply to the riddles of all the sphinxes which are encountered in science; but if they succeed where others have failed, M. Poincaré will be good enough to remember this phrase, and do honor to logistics for the solution.

III.

I now come to the special criticisms that M. Poincaré addresses to the logisticians on the subject of their philosophy of mathematics and, in particular, of their theory of whole number. In the first place, there are certain arguments which it is astonishing to find him using, but which fortunately are not likely to impress philosophers. For example: "The definitions of number are very numerous and very varied....If one of them were satisfactory, no new ones would be given." The same objection might be urged not only to every philosophical speculation—and that is the usual argument of sceptics and positivists—but to every scientific *theory*; M. Poincaré knows this quite well. If this argument had any value, it would be the negation of all progress, even scientific progress. In mathematics in particular there exist numerous definitions of the irrational number, of the limit, of the definite integral, and so on. Has ever any one concluded from this that all these definitions are bad? Certainly not, but simply that certain ones are better than the others, without these others being properly speaking defective or wrong. For the rest, if this argument were to be taken literally, it would prove at the

outside that all the definitions proposed are bad *except one,* the last. Consequently, the argument has no bearing against Mr Russell's definition as long as this definition is the last proposed.

M. Poincaré is surprised that the logisticians define arithmetical addition by means of logical addition which appears to him to rest on an act of intuition which is analogous but "more complex." But in the first place if an act of intuition is really necessary for one or the other of these operations, is it not advantageous and meritorious to define the one by the other, so as to reduce to a minimum the number of acts of intuition? Logical addition is not an invention of the logisticians; it has existed at all times and in all minds. It is the combination which the conjunction *and* expresses in the phrases, " the French and the English," "philosophers and mathematicians." Logic, even classical logic, cannot dispense with it. Thus it is not arbitrarily, as M. Poincaré seems to believe, that this notion is introduced "into the chapter headed 'Logic.' " Given that it is indispensable to logic, the whole question is to know if it can be used to define arithmetical addition. This idea is too natural for Peano and Russell to have been the first to do it; it is already clearly expressed in the work of Lambert. To refute it, M. Poincaré ought to have shown how and why arithmetical addition cannot be defined by means of logical addition, and consequently ought to have criticized Whitehead's[35] formal definition of it. Or, if he believes that the notion of logical addition is "more complex" than that of arithmetical addition, he should try to define the first by means of the second. That is the best means of proving that mathematics is independent of logic. Meanwhile, he ought to allow the logisticians to observe the classical precept that principles must not be multiplied without necessity.

[35] *Amer. Journ. of Math.,* Vol. XXIV, 1902.

M. Poincaré solemnly accuses the logisticians of having violated two rules of method. The first consists in this: Every mathematical definition supposes the existence of the object defined and is only valid on this condition. But this condition is by no means a necessary rule. It is useless to invoke the opinion of John Stuart Mill, whose authority is rather mediocre in the logic of mathematics. The condition that M. Poincare wishes to impose on logisticians is absolutely gratuitous and is not acknowledged by the most rigorous mathematicians. A definition is no more than the giving of a name; it by no means supposes the existence of its object. We can very well define a problematical object, and then prove that it does not exist. Thus Euclid denotes by a certain sign "the greatest prime number," and then demonstrates that it does not exist. We define the derivative or the integral of a function in general, without supposing that every function has a derivative or an integral. What M. Poincaré wished to say or ought to have said is, on the other hand, that a definition does not *imply* the existence of the object defined, and this existence must be proved or postulated if we wish to be able to use it in further reasonings. This[36] is a well-known rule of mathematical method, and it is enough to run through Peano's *Formulaire* to see that each definition is accompanied, when there is occasion, by an existence-theorem which usually determines the conditions under which the object defined exists.

M. Poincaré says that "in mathematics the word *exists* can only have one meaning, it means '*is exempt from contradiction.*' " I am sorry to contradict him on so elementary and essential a point: logical—or mathematical, it is all one—existence is quite another thing from the absence of contradiction.[37] It consists in the fact that a class is not

[36] Cf. *Les Principes*, 39.

[37] It is a curious thing that this conception of logical existence only appears admissible in a *panlogism* analogous to that of Leibniz, and where the exten-

empty; that is to say that at least one member of it exists, and this means by definition that the class in question is not null. It is exactly for that reason that it is the custom of mathematicians to prove the existence of a class by giving an *example,* that is to say by indicating an individual which belongs to this class; and they have no other means for proving an existence-theorem—unless they reduce it to a preceding theorem or postulate of existence.[38]

But, it will be said, how is the existence of the individual which is used as an example proved? Must not this existence be established in order that the existence of the class of which it is a part may be deduced? Although this assertion may seem paradoxical, the existence of an individual as such is not demonstrated. The individuals, by the mere fact that they are individuals, are always considered as existing; or rather the question does not arise for them since logical existence is a property of classes and not of individuals.[39] We never have to express that an individual exists, absolutely speaking, but only that it exists in a class, that is to say, is an element of it.[40] When an individual is defined by means of general terms, its existence is demonstrated in two stages: This individual being defined as *the* u (u being a certain class), we demonstrate that the class u is not null, and then that it is a singular class. The defi-

sion of concepts would be absolutely determined by their comprehension. For example, Leibniz and his disciples believed that if "No man is a stone," that is to say, if no "men-stones" exist, it is because the concepts *man* and *stone* respectively contain contradictory elements (such as *living* and *not-living*).

[On the following discussion of the "existence" of classes and individuals, cf. my remarks in *The Monist,* Jan., 1910, Vol. XX., pp. 113-116.—Tr.]

[38] It is enough to have a proposition of the form: "x is a member of u," to be able to conclude that the class u exists.

[39] Of course, classes themselves can be considered as individuals with respect to classes of classes, but then they "exist" even when they are null.

[40] M. Poincaré thinks it necessary to add to Peano's postulate the following: "Every integer has one which follows it." He does not see that this postulate, which he believes new, is contained in the third postulate: "The consecutive of an integer is an integer." In fact, this implies that the consecutive referred to exists as an individual of a class, and even that it is unique, for otherwise we would say that the consecutives are *contained in* the class of integers.

nition of the individual is then justified.[41] But what we really demonstrate is not the existence of the individual as such but the existence of the class to which it belongs.[42]

In all of this there is no question of contradiction. What then is the relation between the existence of a class and the absence of contradiction in its definition? It consists in this: If a definition is contradictory, *no* individual fulfils its conditions, and consequently the corresponding class does not exist. Contradiction is then a purely negative criterion of existence; it is the criterion of non-existence. And reciprocally, if a class exists, that is to say contains an element, we can conclude from that, as M. Poincaré says, that its definition is not contradictory. Thus existence appears as the criterion of non-contradiction. But it is to be noticed that the relation between existence and contradiction is exactly the inverse of that which M. Poincaré affirms; it is not non-contradiction that proves existence, but it is existence that proves non-contradiction.[43]

It is then arbitrary and misleading to maintain that a definition is only valid if we first prove that it is not contradictory. Besides, it would be interesting to know how we could prove *directly* that a definition or a system of postulates is not contradictory. The presence of a contradiction can certainly be proved, but the absence of every contradiction is, like the innocence of an accused person, a negative fact which cannot be proved directly. Hilbert

[41] It must be noticed that, though the definition of a class has no need of justification (as this class may be null) the definition of an individual must be justified by the double demonstration of the *existence* and *uniqueness*. There is, then, no contradiction here.

[42] Cf. Russell, "The Existential Import of Propositions," *Mind*, July, 1905.

[43] After having written these lines, I found the same doctrine stated by G. Frege in his *Grundlagen der Arithmetik*, §§ 94, 95 (1884). "A concept is admissible, even when its marks contain a contradiction; only we must not suppose that anything falls in its extension. But from the mere fact that a concept does not contain a contradiction, we cannot conclude that something falls in its extension...." (§ 94). "The non-contradiction of a concept can only be established rigorously if we prove that something falls in its extension. The inverse would be an error" (§ 95).

stated in 1900 that we can find a direct demonstration of the compatibility of the axioms of arithmetic;[44] and in 1904 he believed that he had found such a demonstration.[45] But this demonstration is not satisfactory, in the opinion of M. Poincaré himself. If "M. Hilbert hides himself," it is not "because the difficulty is too great," but because the problems which he has proposed to himself appear insoluble. M. Padoa[46] has already replied to Hilbert by recalling that, in his own theory of algebraic numbers,[47] he has demonstrated, by the exemplary method which is the only one possible, the irreducibility of his postulates and their reciprocal independence. And he concluded with this phrase: "The contradictions or the dependencies of propositions can only be demonstrated by deductive reasoning while non-contradiction or independencies of propositions can only be demonstrated by verifications (we verify that properly chosen interpretation of the symbols satisfy or do not satisfy the propositions in question)." In fact, a contradiction or a dependence is translated by a proposition of non-existence or by an implication; while a non-contradiction or an independence is translated by a proposition of existence or by a non-implication. And this difference is equivalent to that of universal and particular propositions in classical logic. We know that we can only establish really universal propositions by demonstration, but that to establish a particular proposition it is enough to cite a single case in which it is true. In general we have no other means, for we cannot deduce it from universal premises without the adjunction of some particular proposition.

[44] Communication to the second international congress of mathematicians at Paris in 1900; cf. *Bulletin of the Amer. Math. Soc.*, 1902.

[45] Communication to the third congress at Heidelberg in 1904; cf. *Monist*, July, 1905. [Hilbert's paper is reprinted on pp. 263-279 of the third edition of his *Grundlagen der Geometrie*, published at Leipsic and Berlin in 1909].

[46] "Le problème no. 2 de M. D. Hilbert," *L'Enseignement mathématique*, Vol. V, 1903, pp. 85-91.

[47] *Bibliothèque du (1er) Congrès int. de Philosophie*, Vol. III, pp. 309-365; *Revue de mathématiques*, Vol. VII, 1901, pp. 73-84.

Now, just as it is impossible to deduce a particular from universal premises—that is to say a negation from many affirmations—it is impossible to prove deductively an existence or a non-implication if we set out from non-existences or from implication. Thus the direct method that Hilbert and Poincaré recommend is impracticable. M. Poincaré has no right, then, to require of the logistician a demonstration which Hilbert could not furnish. He might just as well convict them of impotence by summoning them to take a bite out of the moon.

In default of a direct demonstration M. Poincaré suggests a very curious method of verification. To prove that a system of postulates is not contradictory, it would be necessary, according to him, to compare two by two all their consequences to prove that "there are not two which are contradictory to each other." But, as he himself immediately recognizes, this method is impracticable if the consequences to be examined are infinite in number, as is the case in arithmetic. I add that, in fact, it has never been applied. Nobody has ever seen a mathematician spend his time in comparing among themselves all the propositions of a theory to assure himself that the definitions from which he started do not contain some contradiction, and that *consequently* the entities defined really exist. Where would we be if we had to make such a verification for each new definition? But, it would be replied, it is the whole of mathematics which constitutes this verification; it is a fact that no contradiction between any two propositions has ever been met. Very well, but this verification *a posteriori* is as valid for logistics as for mathematics, since logistics merely claims to formulate the primitive principles and definitions of mathematics. For example, we may ask if the postulates by which whole number is defined are not contradictory. Logistics has only to reply: I deduce from them all the theorems of classical arithmetic; you have

never found the least contradiction in these theorems when you made them rest on vague and confused intuitions; why do you wish that there should be any more contradiction in them at the present time? They are the same propositions, merely reduced deductively to some principles. In any case, the burden of proof falls on those who believe that these principles are contradictory; for contradiction may be proved, but non-contradiction may not.

The method in question is not only practicably inapplicable and unapplied in fact: it is logically illegitimate. In fact, it is not enough to bring two propositions together, to discover that they are contradictory, unless the contradiction is formal and explicit. For example, there is no formal contradiction between the two propositions: "ABCD is a non-rectangular parallelogram" and "ABCD is a quadrilateral which can be inscribed in a circle"; the contradiction only appears when we know the properties of the inscribable quadrilateral, that is to say, when the consequences of the second proposition are deduced. To bring to light the implicit and latent contradiction of two postulates, it would be necessary, accordingly, to deduce all the possible consequences (in number infinite) from these postulates. That presupposes the following definition: "Two propositions are contradictory to one another when they have consequences which are contradictory to one another." But such a definition is illogical because it contains a circle. Thus M. Poincaré's criterion of non-contradiction implies, not merely an infinite regress, but also a vicious circle.

M. Poincaré well knows that the method which he proposes is impracticable. He tries to correct it by means of the principle of induction: "Perhaps there may be a means of showing that a new reasoning cannot introduce contradiction, provided that we suppose that, in the series of preceding reasonings, we have hitherto met with none." Notice the very doubtful form in which this hypothesis is

stated. Indeed, it is a hypothesis in the air, which rests on no example and on no precedent, and which seems to be invented merely to charge the logisticians with a vicious circle. Now, not only is it not true, that is to say, nobody has ever used so strange a kind of reasoning, but it is improbable. To show this, let us see what further indications M. Poincaré gives. He supposes that "a series of syllogisms" can be formed from the starting-point of the axioms as premises; then, "when we have finished the nth syllogism, we see that we can make a $(n+1)$th"; lastly, we can show that, "if there has been no contradiction at the nth syllogism, there will not be at the $(n+1)$th." All these hypotheses are absolutely gratuitous and contrary to all probability. In the first place, mathematical reasonings do not, in general, consist in a *linear* series of syllogisms; otherwise the type of mathematical reasoning would be the *sorites*. Must we repeat that the syllogism is by no means the only type of deduction, and that there are many other logical principles or rules which enter into reasoning? Then, the simple deductions which compose a reasoning to not arrange themselves in a linear series, as M. Poincaré imagines; the image of mathematical reasoning is not a chain but rather a genealogical tree.[48] What, then, does the *number* of reasonings made at a given moment signify if their linear order is always more or less arbitrary, and arises solely from the practical necessity of enunciating them in speech, because time has only one dimension? "The number n serves to count a series of successive operations," says M. Poincaré; what becomes of his argument if these operations are not successive or are only so by accident? Can we affirm that this number n exists at each instant and that it is well determined? We can count simple deductions if they all reduce to the type of the syllogism; but

[48] Cf. *Les Principes*, p. 286 [and the long note on p. 238 of M. Couturat's article, which we have not here translated. It contains the detailed writing out of a simple theorem in the mathematical logic of Peano].

how are we to count heterogeneous deductions which proceed from various rules? Will it be said that each application of a logical principle constitutes a unity? But, besides the fact that all the principles have not the same deductive importance, it can happen that many principles occur at once in an elementary deduction. That is what happens, notably, when one intervenes as premise and the other as a rule of deduction. All this proves that the *number* of deductions, whether syllogistic or not, has no objective reality, and that any numbering of them is arbitrary. Consequently, how are we to admit that a proposition depending on this number can be established and concluded from n to $n+1$? And then M. Poincaré relies on his hypothetical case to attribute to the logisticians a vicious circle which they have not committed. "With an *if*," says common sense, "we could put Paris in a bottle." It is with an *if* that M. Poincaré arrives at attributing a paralogism to the logisticians.

Unfortunately, M. Poincaré seems to forget elsewhere all his *if*'s when he asserts categorically that the principle of induction is necessarily used in every demonstration of the compatibility of the axioms of arithmetic or of any system of axioms. For example, he says: "We must have recourse to processes of demonstration where, in general, we have to use the same principle of complete induction which is the one to be verified." Would it not be believed that the fantastic method which he proposes is in current use? Elsewhere he cites it as one of the "possible applications of the principle of induction," as if this application had been actually made. Finally he says, on the subject of the theorem of Bernstein: "If ever another demonstration is invented, it must still rest on this principle, since the possible consequences of the axioms which are to be shown to be non-contradictory are infinite in number." Thus, it is enough that we have to do with an infinity of propositions

(or of any objects) in order that, according to M. Poincare, the principle of induction *necessarily* intervenes. He has quite forgotten that the application of this principle proposed by him is subject to extremely restrictive hypotheses.

At the bottom, he seems to confuse mathematical induction with induction pure and simple. For how are we to conceive that from the absence of contradiction in a series of reasonings we can infer the absence of contradiction in the following reasonings? Without doubt, if this inference was certain and could be expressed by the precise formula: "If no contradiction has been found in the first n reasonings it will not be found in the $n+1$ first ones," there would be an occasion to apply the principle of induction, and the conclusion would be equally certain. But the inference in question can at most only be probable, and consequently it only constitutes a common induction and not a mathematical induction. To borrow an example from M. Poincaré, the geometry of Lobachevski, since it only comprises a finite number of theorems, did not absolutely prove that the postulate of Euclid is independent of the other geometrical axioms (that is to say that its negation is compatible with them); it only gave this proposition a probability which was greater as the number of theorems of the new geometry became greater. But there is always an abyss between a probability, however great it may be, and an apodictic certainty. Now, the results of common induction are characterized by probability, while mathematical induction is a rigorous process which engenders certainty. If then the inference that is drawn from reasonings already made to future reasonings has only a probable value (as common sense—that "sure instinct" to which M. Poincare refers—thinks), it rests on an induction pure and simple and not on the principle of mathematical induction.[49]

[49] [The fourth section of M. Couturat's paper occupies pp. 241-247, and contains a detailed refutation of M. Poincaré's remark that Mr. Russell had not demonstrated the existence of the integers. M. Poincaré's opinion rested

The second principal fault with which M. Poincaré charges the logisticians consists in that they surreptitiously change a definition: "You give a subtle definition of number and then you think nothing more about it. . . . and when the word 'number' is found farther on, you attach the same meaning to it as the first comer would. . . . Here is a word of which we have given an explicit definition A. We then make use of it, in discourse, in such a way that it implicitly supposes another definition B." That is a very serious reproach that must not be urged without proof against logicians so rigorous and so practised as Peano and his collaborators. Now M. Poincaré gives no proof and confines himself to general reflections on method which affect logisticians less than anybody else, for there is continually in these reflections a question of "words" and of "phrases." Mathematicians who reason with words and phrases are doubtless liable to attribute to a term, instead of the meaning assigned to it by its definition, the meaning which current use gives it. But it is exactly to avoid these illogical associations and implications that the logisticians use symbols whose meaning is solely determined by their formal relations, and which are manipulated in virtue of formal rules of calculation. Has M. Poincaré already forgotten that he reproached logisticians with reducing reasoning to a blind mechanism, that is to say, with neglecting the meaning of their symbols? "To demonstrate a theorem it is not necessary nor even useful to know what it means"; "the mathematician has no need of understanding what he

partly on a misreading and partly on the fact that, in M. Couturat's popular book, the question of existence was rather neglected in comparison with Mr. Russell's work. However, in Mr. Russell's early work, while *existence* was treated at length, the far more important question of *entity* was not considered. Thus the justification of Mr. Russell's early existence-theorems does not now appear to be quite satisfactory, and accordingly is here left untranslated. The second part of the fourth section is also untranslated here. It contains a refutation of M. Poincaré's hasty judgment that the principle of induction is not the definition of finite number, and is slightly more technical than the rest of M. Couturat's paper. What follows is, in essentials, M. Couturat's fifth section.]

does." The two reproaches are contradictory; let M. Poincaré leave to the logisticians at least the advantage of "the logical correction of reasonings" which compensates for its "formal" and almost "unintelligent" character. In any case, all the general and vague reasons which he alleges to support his criticism return against it, for they tend to prove that the logisticians are exempt from the causes of error which he points out.

I have long sought in the articles of M. Poincaré for the precise proofs of his accusation. I believe that I have found one, and yet I am not quite sure. M. Poincaré reproaches Mr. Russell with using two different formulae of the principle of induction, and with confusing them illegitimately: "A number may be defined by recurrence; on this number we may reason by recurrence: these are two distinct propositions. The principle of induction does not teach us that the first is true, it teaches us that the first implies the second." He says again: "The principle of induction does not mean that every whole number can be obtained by successive additions; it means that, for all the numbers that can be obtained by successive additions, we can demonstrate any property by recurrence." In the first place, the expression "successive additions" is not precise. The question necessarily arises, "How many additions?", and the reply is, "a finite number"; but the finite numbers are characterized by the principle of induction. Consequently, M. Poincaré's proposed enunciation means: "For all the numbers which can be defined by recurrence (or by complete induction), we can demonstrate a property by recurrence." Now that is a wholly analytical proposition, and almost a tautology: "All the numbers which verify the principle of induction verify the principle of induction." If this were the formula of the principle of induction, it would be an analytic judgment, and not a synthetic judgment as M. Poincaré maintains.

But that is not the true formula of the principle of induction, and it is incomprehensible how a mathematician like M. Poincaré could have made such a mistake. It is not with him just an airy remark, for he returns to this important question at the end of his second article and gives precise expression to his thoughts in the following terms: "A whole number is that which can be obtained by successive additions, it is that which can be defined by recurrence.... A whole number is that on which we can reason by recurrence....The two definitions are not identical; without doubt they are equivalent, but they are so in virtue of a synthetic *a priori* judgment; we cannot pass from one to the other by purely logical processes."[50]

Will it be said that the logisticians have invented a new enunciation of the principle of induction, which they set up against the classical enunciation? By no means, they have only translated the traditional enunciation into symbols. And what is still stronger, M. Poincaré himself quoted this traditional enunciation at the beginning of his first article: "We know the enunciation of this principle. If a property is true of a number 1,[51] and if we establish that it is true of $n+1$ provided that it is true of n, it will be true of all the whole numbers." Now that is one of the verbal translations of the formula of the principle of induction.[52] M. Poincaré cannot then dispute the exactness of the symbolic formula. Thus he accuses the logisticians of surreptitiously changing a definition; and it is he himself who, in one and the same article, changes the definition, or rather the enunciation, of the principle of induction!

[50] [M. Couturat, on p. 249 of his article, formulates these two definitions in symbols, and shows that the passage from one to the other is effected by a process as analytic as the passage from the proposition, "Pompey is one of the x's such that Cæsar conquered x," to the proposition, "Pompey was conquered by Cæsar," or, "Cæsar conquered Pompey."]

[51] Or of the number 0; that comes to the same thing here.

[52] It is one of the verbal translations that I have given in *Les Principes*, p. 55.

To prove his accusation he himself commits the paralogism which he wrongly attributes to them, and all his reproaches of illogicality fall on himself alone. If I had the wit of M. Poincaré, I would say that his "adventure" is quite as instructive as that of M. Burali-Forti, and that it ought to "warn" the adversaries of logistics of the necessity of being circumspect.

I will not bring up the conclusion of the articles of M. Poincaré because I do not see the utility of carrying the discussion into history where it is complicated by questions of interpretation. The controversy is not "between Kant and Leibniz,"[53] but between M. Poincaré and the logisticians. Besides, the question, as M. Poincaré has put it, is not a question of general philosophy or of epistemology, but of pure logic. Admitting the principles and the primitive ideas of the logisticians, M. Poincaré has maintained that, setting out from these data, they cannot build up mathematics without another postulate—an appeal to intuition or a synthetic *a priori* judgment; and he has thought that he has discovered in their *logical* construction certain paralogisms (beggings of the question or vicious circles). I believe that I can conclude from the above discussion that not one of these theses is proved, and that, in particular, the logisticians have not committed any of the logical errors that are so lightly imputed to them. I have too high an idea of the wit and the character of M. Poincaré not to believe that he will form a more just and more favorable opinion of logistics...when he has studied it.

LOUIS COUTURAT.

PARIS, FRANCE.

[53] [*Monist*, April, 1912, Vol. XXII, p. 256.]